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ON THE 'LANGUAGE OF OBSERVATION' IN LINGUISTICS*

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ABSTRACT

Use of "the language of observation" as a level of scientific discourse is exemplified. The formal properties of this language are characterized, and the derivability and feasibility of the axiomatic method as applied to it are considered. Also discussed are the theoretical and practical significance of this level with respect to various linguistic (including universal) statements, the seemingly relative nature of observational and theoretic levels, and the question of alternative types of explanations.

*This is a revised version of a paper presented at the Conference on Languages of the World held at the Center for Applied Linguistics in April 1970.

Encountering such an expression as 'the language of observation', the linguist will immediately conclude that the 'language' in question is not a natural language, but one of those artificial postulational constructions typically produced by philosophers of an analytic bent. Such 'languages' are generally formalized and their value is presumed to lie in the clarification of concepts which result, or at any rate are intended to result from the use of formalization.

In the present instance, however, we will be concerned with something too inchoate to submit easily at the present time to formalization. The justification, however, for using a term with 'formal language' overtones, is that we will not be describing natural languages directly but will rather be concerned with the analysis of a certain kind of discourse which occurs in the shop-talk of linguists themselves, something perhaps not unlike the 'language stratum' of certain ordinary language philosophers.

The purpose in doing this is to call attention to the existence of such a stratum of discourse, to assay its significance in overall linguistic theory and to suggest lines along which a more systematic treatment might eventually proceed.

In spite of a certain vagueness which seems inseparable from the subject in its present state and the inconclusive nature of much of the present argument, the questions raised here are, I think, worth raising. Moreover, notwithstanding their rather rarefied and abstract nature, they relate fairly directly to the general topic of languages of the world with which this conference is concerned. For such an enterprise, a central question is the comparability of linguistic descriptions constructed in accordance with differing theoretical models. If there is, indeed, an underlying language of observation in the sense to be discussed here, then one of the essential functions that it can fulfill is to provide a universal basis for the comparability of language descriptions.

In fact such a basic language exists in practice and is constantly utilized but without receiving explicit recognition. The following is an illustrative example.

In one of the introductory chapters of Emmon Bach's excellent book, An Introduction to Transformational Grammars, the student is presented with two alternative descriptions of the phonological system of a hypothetical language X, labelled Description 1 and Description 2, respectively.

These two descriptions are formalized in the sense that they are presented, largely in Description 1 and completely in Description 2, by sequences of symbols rather than words in ordinary language. In neither instance is the description discursive. That is, they do not consist of sequences of sentences with syntax structure of ordinary English sentences, which would still be possible even with the employment of a technical terminology couched in symbols. Bach then adduces arguments to show that Description 2, which consists of ordered rewrite rules, is superior to Description 1, which consists of unordered 'taxonomic' statements.

The linguist has become so inured to formalized modes of expression that unless it has been specifically pointed out, he will probably not have realized in reading Bach's text that, in fact, not two but three formulations have been presented. One of these three has not been explicitly recognized, nor is it in the running in the debate regarding the relative merits of Description 1 and Description 2.

The third, silent contender is a discursive verbal description of language X which is presumably there because without it the elementary student for whom the book is intended would, no doubt, find Descriptions 1 and 2 unintelligible. Even the professional would probably find this discursive description of some assistance and he certainly would be unable to understand Description 1 and 2 without this verbal prelude, had he not in his previous training mastered a technical terminology on the basis of statements explaining them in ordinary English. The situation here is no different in principle from the introduction of basic mathematical concepts or, more generally, scientific concepts, in ordinary language which must have been mastered before it becomes possible to understand the technical languages of mathematics and the sciences.

Bach himself evidently considers this initial discursive statement as different in some fundamental way from the rival "scientific" descriptions which follow. Thus, he prefaces his consideration of language X with the following remarks (p. 20): "I shall state the facts for the language in ordinary English and then give two descriptions, the first in a form familiar to students in modern linguistcs, the second as a set of rewrite rules."

The example just given resembles many others that might have been cited in the following respect. The possibility of such factual-level statements is taken for granted and utilized in practice without according it any explicit theoretical recognition. The point of view that I will seek to outline in a very preliminary fashion here is that the existence of this level and its characterization is a matter of some theoretical significance. It goes well beyond its pragmatic contribution on the psychological side to the intelligibility of higher-level discourse in that it is related in a fundamental way to two of the basic criteria of scientific theory construction, empirical verifiability and completeness.

It is generally agreed that any scientific description must be rejected if it has as a consequence the logical derivation of consequences which are in conflict with observed fact or if all the facts are not accounted for in the theory. The foregoing statement requires certain qualifications. First, agreement with fact is relative to a somewhat vague norm in accordance with which certain violations are tolerated if they are sporadic, or, where the observations are quantitative, if there is reasonably close approximation to the theoretically expected values. Still, gross violations are not easily accepted and even small or sporadic deviations can eventually lead to revision of theory if it can be shown that these deviations can themselves be accounted for systematically, e.g. Verner's law as accounting for the exceptions to Grimm's law.

The requirement of completeness also calls for some comment. It does not necessarily refer to the language as a whole. It can, and in practice most often does, relate to some subpart defined in advance of the investigation, e.g. the system of verbal conjugation, the segmental phonologic system, etc. Such presystematically drawn boundaries are often vague at certain points but there is a body of data which by general agreement must be accounted for.

Besides these two criteria of agreement with fact and completeness, that of generality with its much discussed and evidently complex relation to simplicity is the third basic criterion. Yet the former two notions seem to have a kind of priority in that a description which violates them, however well endowed in respect of elegance, simplicity, generalizing power, and insightfulness, will be subject to rejection.

This level will hereafter be referred to as O, the language of observation. It is far broader in scope than appears in the foregoing example in that it is not confined to phonology, to the writings of a single school in linguistics or to the sole purpose of making clear for pedagogical reasons for elementary students the underlying facts which are being subjected to theoretical analysis.

Thus, there are numerous examples in the literature of differing analyses of morphological systems, e.g. that of the RUSSIAN verb conjugation in terms of alternative models such as traditional paradigms, morphemes, and their distribution, or generatively in which all the participants are in essential agreement regarding the 'facts'.

As appears from such examples, and also from that in Bach's book, in some way O seems to provide a language which is neutral in relation to the various schools and their rival analyses, or between members of the same school when presenting different analyses. Thus Bierwisch and Ross, both members of the TG school, present differing theories regarding underlying word order of subject, verb, and object in GERMAN. However, both agree that the basic facts which have to be accounted for are that in main clauses GERMAN has 'normal' SVO order, and in subordinate clauses SOV. Once again these facts are taken for granted as an area of agreement by the participants, and the facts in question are of the kind that can be learned from standard grammars of GERMAN which make no particular claim to theoretical sophistication.

As we have seen, a striking element common to all these instances is lack of explicit reference to O as a level. It is a kind of background phenomenon apparently never brought into the forefront of consciousness. The closest to overt recognition in the context of contemporary theory is probably in Chomsky's well-known theory of the three adequacies, though even here, as elsewhere, it characteristically lurks in the background. For the first of the three adequacies, 'observational adequacy' would seem to have no locus in reality unless there were statements embodying observed 'facts' to which we might refer in evaluating the observational adequacy of a description.

Let us grant, then, the existence of such a level of factual statements based on observation of the actual practice of linguists and involving, it would seem, a rather surprising consensus in the specific data even among linguists of different schools. How can this level be characterized from a theoretic point of view and whence does this consensus arise? Perhaps we may, for purposes of initial discussion, characterize the kind of discourse with which we are dealing in the following way. It consists of statements regarding individual languages which rest in some reasonably direct way on a body of observations, that is, it consists of 'observation statements'. The difficulty, however, of distinguishing descriptive from purely observational statements is notorious, if indeed the latter exist at all. What is being aimed at can be expressed by two criteria: (1) particularity, i.e. absence of generalization, (2) the use of terms based directly on physically observable characteristics which are not defined by means of logically more primitive concepts within some theory. That statements presented as factual in linguistic discussion do not satisfy these criteria any more than in another science will soon appear and is, in fact, not at all surprising.

The general point is indeed far from novel. Thomas Kuhn states it cogently in his well-known work, The Structure of Scientific Revolutions. Alluding to the 'natural history approach' he notes (pp. 16-17): "No natural history can be interpreted in the absence of at least some implicit theoretical and methodological belief that permits selection, evaluation, and criticism." We may revert for illustrative purposes to the example from Bach's book, An Introduction to Transformational Grammars cited earlier. The following are two of the statements included in his account which, as we recall, is stated to be "facts for the language in ordinary English."

1. "Every syllable consists of a nucleus, an optional string of consonants, and an optional final consonant."

2. "The phonemes have the following conditional variants (allophones): /p k/ are voiced between vowels, /a/ is [o] except where stressed."

Regarding the first of the two ideal criteria for observation, lack of generality, the very occurrence of the word 'every' in the first statement, "every syllable . . ." is an overt indication of generality. Regarding the second criterion, the use of terms based directly on observation and not definable by means of more primitive terms in some theory, both statements abound in terms which are not 'primitive' and are defined within a framework of theory. This is particularly striking in the second statement with such giveaway terms as allophone, phoneme, and the technical use of brackets for the first and slashes for the second. Thus, to spell out the obvious, 'allophone' is a theoretical concept which presupposes such other concepts as environment and phonetic similarity, which in turn presuppose 'feature' in the traditional phonetic sense. If two phones are similar they must be similar in certain "respects" stateable in phonetic terminology. Moreover, 'allophone' is itself a relational term involving in its own definition the term 'phoneme'. There is no such thing as an allophone in isolation. It is always an allophone of some phoneme.

While, in light of the previous remarks regarding the nonexistence of pure observational statements in science, these results are in general not unexpected, one aspect does seem worthy of being noted. The body of theory presupposed by these 'statements' of fact is essentially classical phonemic theory. Thus, as so often in the history of human thought, the theoretical statement of yesterday is the factual observation of today. One is reminded of Bloomfield's remarks concerning the "nature of such terms as subject, object, and predicate" (Language, p. 3): "Like much else that masquerades as common sense, it is in fact highly sophisticated and derives, at no great distance, from the speculations of ancient and medieval philosophers." In the present instance, however, the time interval is to be measured in decades rather than centuries.

It should be understood that the foregoing remarks are not intended in any fashion as a hostile critique of Bach's excellent and sophisticated book. What Bach has done here is normal for the practicing scientist who is quite capable of moving his subject matter ahead without worrying about the epistemological basis for what, at any particular point in the history of science, passes for its lowest-level observational statements.

What we might, in light of the foregoing example, call the 'relative' nature of the distinction between an observational and a theoretic level, calls for further analysis. Does it not put into doubt the assertion made earlier in this paper that O, the language of observation, provided a common basis which was neutral in relation to differing theoretical schools? If, for example, we consider a typical instance of classical phonemic analysis such as Bloch's analysis of JAPANESE, this relativity becomes particularly clear. Bloch's exposition, in accordance with the pattern being sketched here, and in this case quite overtly, is based on two levels. There are first numerous statements regarding the phones of JAPANESE and their distribution, and the set of these statements obviously constitutes for Bloch a lower 'factual' level which is then accounted for theoretically by assigning the phones to higher-level units, the phonemes. Yet these latter 'theoretical' statements of Bloch could easily, mutatis mutandis, pass muster as factual-level statements in an analysis such as Bach's.

However, examples of this sort do not destroy the notion of an absolute basic level O. The following considerations would seem to apply. It would still be true that from an analysis of JAPANESE based on a model like that of Bach, one could deduce a whole series of statements quite like those of Bloch's lowest-level distributional statements, and that these would in principle be identical. One would here have to distinguish two different contexts of exposition in the transformational generative literature. One of these might be called the pedagogical. The preliminary factual statements by Bach regarding his hypothetical language X is but one example. They abound in such works as Koutsoudas' Writing Transformational Grammars and Harms's Introduction to Phonological Theory. In this latter work the equation, structuralist's theoretical level = transformationalist's factual level, occurs at times in a highly explicit way. The data for an exercise in theoretical analysis is an actual 'phonemic' statement taken from the structuralist literature. In these instances the reduction to the structuralist's factual level involves simply a reversal of the pragmatic order of the structuralist's exposition. The structuralist presents phonetic "facts" and constructs phonemic statements on their basis. From the phonemic statements thus presented the phonetic facts can be recovered. The other context is that of actual grammars. Here the relationship is far more indirect. Nevertheless it is clearly possible to deduce such low-level distributional statements from the total phonology. What is most important,

it is possible, or perhaps more accurately put, there is usually sufficient clarity in the use of theoretical terms to ascertain, given two descriptions (whether from the same or different schools), which differences rest on discrepancies in fact and which on differing theoretical analyses.

The second observation that might be offered in regard to this relativity of theoretical and observational levels is that the lowest-level distributional statements of the structuralist no more satisfy the ideal criteria for pure observation statements than those of Bach with regard to the facts concerning his hypothetical language X. Their generality is obvious, for example, in statements such as: "The phone č never occurs before a, o, and u." Such statements are based on generalizations over an already "edited" corpus. Moreover, there is a framework of theoretical terms, namely those of phonetic theory which exhibit a comprehensive logical organization.

The phenomenon discussed in the foregoing section as exhibiting the relativity of observational and factual levels may be considered, from another point of view, as a difference in linguistic usage by linguists themselves, i.e. their usage of such terms as 'fact' and 'theory'. If we view our task here as a theoretical explication of the term 'fact' as used by linguists, then such discrepant meanings are the normal raw material of differing presystemic usages which are typically encountered in attempts at explication. A preliminary criterion intended to guide such efforts was mentioned earlier, namely, that the language we are seeking to characterize "rests in some reasonably direct way on a body of observation." Such a criterion is meant merely as a guide, and its vagueness as so stated is obvious, yet it has an overriding role, once it has been adopted, as against the uses of such terms as 'fact' by linguists in actual practice. In what follows I shall try to outline one possible approach in terms of which greater precision can be given to this criterion, and in terms of which the relativity of usage just discussed can be accorded its proper place.

One form such a reconstruction might take would be use of the well-known axiomatic method. I do not mean by this that a full axiomatization should be carried out. It quite probably would not prove desirable even if it were feasible. It is rather that a consideration of the problems arising in axiomatization might provide a framework for ordering and clarification.

The axiomatic method involves in its essentials the construction of a calculus which in its initial phase is uninterpreted and in which more complex notions are defined in terms of logically more primitive notions until the most primitive terms are reached which are undefined in the calculus. The terms of this calculus are, of course, not chosen in an arbitrary manner.

Most of them will ultimately, when the calculus is interpreted, stand for concepts which are already used in the working technical vocabulary of the science, and the connection will be exhibited normally by a choice of symbols whose reference to these concepts will be obvious (e.g. letters which abbreviate the usual designation of the concept). Ultimately, the primitive terms receive definitions of a quite different sort, definitions which provide the connection of the whole system with empirical observations. These definitions have traditionally been called coordinating definitions, but since they provide the basis for interpretation of the system as a whole, I shall call them interpretive definitions; in fact, they are not unconnected with the tasks of the interpretive components as set forth in contemporary transformational-generative grammatical theory. These two parts of the theory, the calculus itself and the interpretive definitions, involve rather different considerations so that it will be convenient to refer to them as C and I respectively.

The notion of C provides a convenient method of accounting for the relativity of levels discussed earlier. However differently either the word 'fact' or some synonymous expression is being used by different linguists or, what is more frequent, however different the types of statements that are being employed as the lowest level, we will say that those concepts and statements provide the axiomatic basis of O from our point of view, which cannot be defined in terms of other concepts. Hence, when Bach uses a term like allophone in what he is in practice treating as lowest level in his discourse, we will consider that these terms are not primitive in this discourse, even though they are not defined explicitly, as long as their understanding can be said reasonably to involve more primitive terms such as environment, phone type, etc. Assuming for the moment an axiomatization in which this reduction takes place, we may say that although Bach uses terms like allophone and phoneme and Bloch uses such terms as environment and phone, they are, so to speak, 'speaking the same language' insofar as the underlying axiomatization might well turn out to be identical. I shall call this reduction to lower-level concepts resolvability. The thesis of a common O advanced earlier would then come to this: that there is an agreement among linguists regarding the underlying calculus of axiomatization, were this axiomatization to be carried out.

Undoubtedly the discussion so far has tended to a kind of optimistic overstatement which I shall now try to counteract to some extent, and in the process bring in considerations inextricably connected with I. This accentuation of the positive, which was purposeful, seems justified since the central point, which I believe to be a valid one, is that there is, in general, a common basis of observation statements on which linguists agree and which they seek to account for in various manners by higher-level theories.

One source of this optimism has been the concentration in the present discussion on phonology, obviously an area of relatively high agreement in regard to observation statements. Yet even here it would, of course, be incorrect to assert complete unanimity regarding such a level. There are two major areas of indeterminacy, one relating to I and the other to C. In regard to I, we may distinguish two main types of interpretation which may be called physical and phenomenological, respectively. By physical interpretation rules are meant those in which it is possible to specify in a reasonably satisfactory way either physiological or acoustic parameters. In other instances we are not in a position to do so. What we have is rather the kind of intersubjective agreement based on perception found in everyday life in relation to such concepts as 'red'. Notions relating to syllabicity doubtless belong here. In such cases, while there is typically a large area of agreement there may also exist a not inconsiderable range of uncertainty. In the case of the syllable, a conspicuous instance is the determination of syllable boundaries. Perhaps we should include here still a third class of primitive terms whose interpretation shares a lack of physical specification with the second type, but which probably cannot be reduced to perceptual agreement. Here we might include such shadowy but indispensable concepts as phonological word boundaries. The other major problem area belongs to C. The basic issue here can be conveyed by the question: how fine-grained should the observations be? The area of possible maneuver for any particular language description would seem to have a lower bound in the limits of consistent perceptual discriminability and an upper bound in actual contrastiveness in the language. That the extent of phonetic specification is an unsolved problem is recognized in present-day phonological theory. At the very least, though, it might be pointed out that the relation between finer- and coarser-meshed descriptions is highly structured. Thus, to pursue the mesh metaphor, two descriptions might be called congruent if all the wires of the coarser mesh coincide with some of those in the finer, and such congruence is usually found in practice.

A basic question which has not yet been raised concerns the status of O in regard to the relationship between individual language descriptions and cross-language generalization. In considering the question, it is essential to distinguish several types of discourse, or 'language-strata'. We have considered O, itself, up to now, as containing observation statements regarding specific languages. Such statements clearly belong to a different kind of discourse from that in which concepts occurring in O are reconstructed, whether axiomatically or otherwise. Such a language is essentially a metalanguage for O. Unlike O itself it is nonempirical in that it is entirely postulational in form and consists of analytic sentences, even though the system itself has been devised in such a manner that its terms can be applied to individual language observations. While it consists of definitions and tautologies, it is of course corrigible in the sense that it becomes subject to modification if it ceases to give the desired results when applied to individual natural languages.

In contrast, O itself is synthetic in that by means of the interpretive definitions of Meta-O, its statements receive meaning in individual language statements capable of empirical truth or falsity. That these concepts are intended to be applicable to all languages, not just to certain individual languages, is in general clear. The example of the use of terms like allophone in describing the phonological facts of Bach's hypothetical language X provides a kind of a fortiori evidence for the thesis of general applicability.

The preceding is a particularly straightforward example; such straightforward instances of universal applicability are especially characteristic of phonology, or at any rate, of segmental phonology. Regarding grammar the situation is more complicated in a number of ways. One set of complications arises from the fact that we are dealing here with form and meaning. One type of situation encountered can be illustrated from the description of case systems. Here it would seem that the two aspects lend themselves to two fairly independent types of analyses, corresponding with reasonable exactitude to the traditional areas of morphology and syntax. The first is the organization of the formal structure in which the names of individual cases are as it were mere labels for distinguishable entities abstracted from the semantics of case use, but not from phonological shape. In regard to the second aspect, the semantics of case, the situation is in principle probably parallel to that of phonology in that description could be carried out for individual languages by means of concepts ultimately resolvable into primitive ideas stated in the metalanguage of O and applicable to all languages. However, the difference in practice is that the kind of analysis which could be incorporated into Meta-O has not yet progressed very far. It is, I believe, a result congruent with the proposals of Ferguson in his paper for this meeting,* that the most promising resolution would seem to be in the direction of positing individual 'case uses', e.g. agent, separation, accompaniment as the logical primitives of Meta-O in terms of which such terms as 'instrumental', 'dative', etc., in individual languages could be defined as more complex entities. However, if we compare this situation with that in phonology we see that a comparable achievement would require the distinction of such uses as but a first step. There is, it is true, once more a level O of observation insofar as there are certain facts about case usages in particular languages readily stateable in traditional terminology. What is lacking, when compared with phonology, is an overall descriptive framework, one which for phonology was already provided by traditional phonetics and which systematically defines a universe of possible speech sounds. Viewed in this light, Hjelmslev's study of case was a pioneer attempt to provide just such a systematization.

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*See "Grammatical Categories in Data Collection" in this issue of Working Papers on Language Universals.

The relatively greater complexity of problems in this area is shown by a further class of facts which can probably be cited from any case language and which is exemplified by rules of case government by verbs. Some of these can probably be covered by generalized statements, e.g. 'verbs of wishing', but some are usually the specific property of individual lexical items. These do represent, once more, examples of specific O since they involve facts on which various observers can agree. However, while there is a reasonable expectation that such concepts as 'agent', etc., can be formulated in Meta-O with the potential of cross-linguistic applicability, it would seem that individual lexical items figuring in such rules are irreducibly individual language facts.

The language stratum that might be called generalizing O is that form of discourse in which universals are stated, again using terms definable by means of the primitive ideas of Meta-O. Logically such discourse is empirical like specific language O. Its basic difference is that it contains variables whose values range over individual languages. That there is a body of such generalizing statements in phonology at least, which have thus far received empirical verification, should not be a matter of dispute, although their theoretical significance has been contested by some. Such universals stateable on the basis of O-level facts of individual languages, some unrestricted and others implicational in form, are found in Postal's Aspects of Phonological Theory, and are the basis for the marking conventions in Chomsky and Halle's The Sound Pattern of English.

Exceptionless implicational syntactic and semantic universals arrived at by the straightforward comparison of surface observation are perhaps less widely accepted, though again I would contend that the evidence for them is indisputable. Thus, to cite but one example, languages with a trial number in the pronoun always have the dual. Those which have an inclusive/exclusive distinction in the first person plural have it in the trial, if it exists, and in the dual if it exists. What is perhaps even less often realized is that there are in all probability some unrestricted universals of surface grammar also. For example, I have noted, thus far without exception in a variety of languages of differing fundamental order types, that when a personal pronoun is in apposition with a noun phrase (as in ENGLISH we, the people) the pronoun always precedes. Such examples, should they be verified, have, it would seem, considerable theoretical significance. They are in fact akin to 'conspiracies' noted in recent work regarding individual languages, but differ in that they belong to universal O. To set up, as has been suggested, a metaprinciple distinct from the grammatical rules themselves which would select precisely those alternatives among deeper-level rules which lead as it were to the foreordained surface result would not obscure the fact that the metarule itself is a generalization based on surface observations.

This paper has been devoted essentially to a discussion of the 'language of observation'. Nothing has been said regarding higher-level theoretical discourse. It has received an implicit negative definition in that it would not "rest in some reasonably direct way on a body of observation". That present TG grammar provides such a theoretical level is clear and it is, indeed, one of its great accomplishments. What is often called 'abstractness' of theory refers precisely to the existence of theoretical constructs which do not possess a direct and simple relation to the body of observed facts. Obviously such a vague phrase as 'direct and simple relation' is in need of more careful explanation. Once again the situation in regard to phonology is reasonably clear. In various writings on phonological theory by Halle and Chomsky, certain requirements of classical phonemic theory are rejected which would limit theoretical elements such as phonemes to a relation in which any particular phone would in all its occurrences be assigned to the same phoneme. In traditional phonemic theory, at least as practiced in the United States, which does not allow such overlapping, the relation between phone and phoneme was then a very simple one, merely class membership.

The considerations just adduced are intended merely to give some idea of what is meant by directness and simplicity of relationship. They do not apply with any literalness to grammar, and even for phonology they do not do justice to the variety of structuralist practice which quite often tolerated a fair degree of 'indirectness'. For example, certain phonetic properties of segments, even though contrastive, were often not assigned to the same phoneme as other phonetic properties of the same segment, but rather to junctures which should be considered as true theoretical constructs. Also, the process approach in morphophonemics practiced by some American structuralists involved setting up theoretical base forms whose relation to the observed facts was complex. It is clear, however, that whereas in structuralism all such departures from direct and simple relation to the level of observed fact were resorted to only out of necessity and then usually with some opposition (e.g. Bloch's objections to juncture, and the attempts to maintain item and arrangement models in morphophonemics), TG grammar freely employs theoretical constructs and considers them a virtue rather than a pis aller.

It is not within the scope of this paper to consider developments in linguistics in their relation to the broader trends of scientific methodology. It can be noted incidentally, however, that the criterion for theoretical concepts that they be constructed from observables is often considered the essential characteristics of positivistic approaches. Thus, Harré (Theories and Things, p. 19) summarizing the views of Mach, the classic source of modern positivism, says among other things, "The only propositions which are worth enunciating are those stating concomitances of observables."

It is now clear that theories in physics, the science with which the great majority of the philosophers of science have been concerned, do not have this simple structure. On the other hand, it does not seem reasonable to attempt to impose conclusions derived from the nature of physical theory to linguistics in a literal way. Linguists should rather pursue the kinds of theory that are most fruitful from their point of view without being constrained by the requirement of either conformity to, or transcendence of, positivistic limitations.

It seems to me not unreasonable to assert that historical explanation of the traditional kind possesses the same kind of indirectness in relation to the data of observation as well as explanatory power similar to that of transformational grammar. The resemblance between the two which is generally agreed, however, not to be an identity is therefore not really surprising. Once again, the picture in phonology is reasonably clear, compared with other aspects of language description. Far more work needs to be done on historical syntax, for example, to ascertain to what extent syntactic deep structures represent earlier historically attested surface structures.

In closing, the question might be raised whether these are not, in fact, rival types of explanatory theory. If so, historical explanation in the traditional sense of accounting for surface linguistic facts of t_2 by principles of change (i.e. processes) of lesser or greater degree of generality operating on the surface linguistic facts of t_1 where t_1 preceded t_2 would be a worthy alternative. Recently it has become more and more apparent that there are alternative views of the nature of syntactic deep structure which differ in fundamental ways, with no convincing way at present of choosing among them, e.g. Chomsky, Fillmore, McCawley. Even within what is essentially the same model, differing accounts of the same surface phenomenon occur. Since this situation exists to a far greater degree in syntax and semantics than in phonology, any statement regarding the relationship between historical grammar and deep structure is relative to the particular interpretation of deep structure which is being employed. This point arises repeatedly, for example, in the very thoughtful paper of E. C. Traugott, "Towards a Grammar of Syntactic Change." This relative lack of constraint on theories of deep structure would seem to contrast with the determinateness both of earlier historic states and of the corresponding principle of change involved in the derivation of later states. The ultimate test should, it is submitted, be empirical and rest mainly on their respective abilities to provide a basis for fruitful generalizations concerning language.

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